

Geosci. Model Dev. Discuss., referee comment RC1  
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## **Comment on gmd-2021-173**

Anonymous Referee #1

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Referee comment on "An improved carbon greenhouse gas simulation in GEOS-Chem version 12.1.1" by Beata Bukosa et al., Geosci. Model Dev. Discuss.,  
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A new greenhouse gas simulation in GEOS-Chem is presented, which couples CO<sub>2</sub>, CH<sub>4</sub> and CO. The paper presents a detailed evaluation of the new system and compares the model simulations to observations.

It seems likely that this new framework will be useful for inverse modelling studies in the future. The analysis is very thorough, although perhaps some aspects could be cut or moved to the Supplement to help streamline the text (which is quite long). However, I have some concerns about the experiment design and interpretation. I feel that the paper would be suitable for publication in GMD after these concerns have been addressed.

### General comments

Experiment design: The novel component of the paper is the description of the coupled CO<sub>2</sub>-CO-CH<sub>4</sub> system. However, I found the discussion of the comparisons of the coupled and uncoupled systems very hard to follow in places. I think this is primarily because the various simulations were run with different OH fields and model resolutions (and with/without an OH diurnal cycle, although this doesn't appear to have much impact). To partially address this, an additional OH run is carried out to test the sensitivity to OH,

introducing another model into the mix. It seems that the reason for these inconsistencies is historical, although I wasn't entirely sure from the text. It seems to me that this paper could be dramatically improved, streamlined and clarified by carrying out two sets of model runs, using the same OH field, the same resolution, one run coupled and one uncoupled. Given that it's relatively cheap to run these simulations, I'm not sure why this wasn't done. Can the authors explain? If it's to maintain traceability back to earlier GEOS-Chem simulations, I'd argue that this should be a secondary consideration, compared to showing the improvement due to the new simulation setup.

Comparison to observations: The conclusions state that the new model improves the fit to the observations (L488 – 490). However, I don't think that this can be concluded. Since we do not know the "true" flux magnitude for these gases, we can't be sure that the coupled simulations are really improving the fit to the data, or just compensating for some bias(es) in the flux fields. For example, for methane, it is stated (L446) "The coupled-origOH results show a positive bias with overestimated CH<sub>4</sub> values for all sites; however, using globally more abundant OH fields (v9-01-03) in the coupled simulation resolve this large bias". However, it could well be that the new fields are simply compensating for some bias in the (highly uncertain) emission field.

Specific comments:

L107 (Eq. 2): I don't think this equation works. For closure, I think there also needs to be a term representing the net flux from/to the troposphere.

L113: [OH] has already been defined.

L129 (Eq. 7): Again, need flux to/from the troposphere.

Figure 3: Is this figure relevant?

L226 – 227: Some of this inter-annual variability is present in the uncoupled simulation. Is the change really so marked in the coupled version?

L231 and Figure 5 caption: The figure shows the CO production, not the “Changes of the CO production”. The change can be inferred from the figure, but is not directly shown.

Figure 4: Add an x-axis. Also, it’s not clear why a bar chart is the best way to present this. How about a line graph?

L244: “Hemispheric” seems preferable to “regional” to describe the table.

Figure 7: Perhaps move this, and the discussion, to the Supplement. I’m not sure it adds too much.

L340: I think a comma would be preferable to a semi-colon between "seasons" and "but".

L431: The offset in the modelled values will lead to a small difference in the CO production, etc., compared to the real atmosphere (i.e. CO production from CH<sub>4</sub> will be over-estimated, since the model spin up leads to higher CH<sub>4</sub>). Is this effect important?

L449: "resolves", rather than "resolve" (although, see general comment... I don't agree with this statement!)

L464 / 465: Notwithstanding the issues around flux magnitudes, these differences seem very small compared to all the other uncertainties in the system. I think it'd also be fair to say that the model changes had a negligible impact on the comparison with the observations here.

L488 – 491: I think these lines in particular (and many others throughout) need to be revised in light of my general comment regarding the potential impact of uncertain fluxes.

L593: I don't think you can say that the v5-07-08 fields are incorrect based on this analysis, given the emissions uncertainty.

L607 – 609: As above, I think this conclusion needs to be removed.

Appendix A: Titled as "Appendix A: Appendix A"